Kuhn's unsuccessful revisionism: a rejoinder to John Selby

It is quite true that my original paper did not deal sufficiently with Kuhn's theories to justify my critical charges against them. My principal ambition was to present a decisive empirical study of social scientific change, one whose theoretical "lessons" and relevance would be there for all to see. It is true, nonetheless, that in failing to elaborate my criticisms of Kuhn I assumed a certain perspective which was not demonstrated. Although I have more than made up for this silence elsewhere (Alexander, 1982:Ch. 1), I welcome the opportunity to present the main points of my understanding here, even if in a form that is still all too brief.

My critic acknowledges the (relative) validity of my objections to Kuhn's first and major interpretive work, the 1962 edition of *The Structure of Scientific Revolutions*. Although even my criticisms of this first work were too compact — a condensation which this note intends to loosen — it is the lack of my reference to Kuhn's subsequent work that is the major object of reproach. But if I did not refer to Kuhn's later "second thoughts," "reflections," and "postscripts," it is not because they challenged the understanding of scientific change I laid out. It is rather because they are ultimately unsuccessful attempts at revision which while implicitly acknowledging the weaknesses of which Kuhn stands accused, do not, in the end, relieve his theory of their burden.

All students of science owe Kuhn a debt of gratitude for raising the banner of postpositivist thinking in such an effectively polemical way. Still, Kuhn greatly overstated his case, and in doing so he put the very postpositivist banner he carried in a vulnerable and uncertain position. Indeed, ever since *The Structure of Scientific Revolutions* (Kuhn, 1962) drew its venomous (and often undeserved) criticism from positivist and empiricist quarters, distinguished postpositivists of various stripes — e.g., Toulmin (1972), Holton (1973), Feyerabend (1975), and yes, even Lakatos (1970) — have sought to defend the anti-empiricist position "in spite of" the monograph which has generally been regarded as its very embodiment. They have carried this task out by themselves offering strong criticisms of Kuhn's work, and I am inclined to believe that when this mopping up is finished very little will be left of the original work, though Kuhn's pivotal role in the intellectual history of science studies will, nonetheless, rightly be assured. I am inclined to believe this not only because of the power of such critics' responses — responses which are generally sympathetic to the epistemological position Kuhn himself takes — but also because of the defensive nature of Kuhn's series of replies. Generally, he has responded to criticisms by modifying earlier positions. While he should be given credit for acknowledging his errors, it is nonetheless true that he has rather unsystematically reneged on the very contentions that most distinguished his original position, on those very points that made his theory controversial to begin with. Insofar as his revisions have been successful, then, they have made his position indistinguishable from — and, ironically, less precise and powerful than — those of a number of other postpositivist thinkers who have criticized his work. This is a point my critic does not seem to recognize.

The second point my critic ignores, however, is more significant: in his effort at revision, Kuhn does not go nearly far enough. His "new" theory — insofar as it is a theory at all — retains many of the central problems of the old. I will try to demonstrate this debilitating continuity in the short space that remains, and I will do so in reference to the famous "Postscript" to the second edition of Kuhn's major work.

The critical issue in discussing Kuhn's relevance for the social sciences is not whether or not the social sciences have paradigms, but, much more fundamentally, what Kuhn means by the concept of paradigm itself. The central problem that Kuhn deals with, and it is a central one to any sociology of knowledge, is the relation between groups and ideas. Included within this essential problematic are two subsidiary questions. Is the group which carries a set of ideas internally cohesive? Are the ideas which are so carried internally consistent as well? In his original work, Kuhn insisted that both these subsidiary questions should be answered affirmatively:paradigm groups are consensual and the ideas that constitute paradigms are all of a piece (or, to put the latter issue in terms of my article, Kuhn contended that the different elements of a paradigmatic scientific theory are intrinsically connected to one another). It is this group consensus and ideational cohesion that explains — in theoretical, not empirical terms — the reason for Kuhn's confidence in the revolutionary character of major scientific change. If a principal element of a paradigm is successfully challenged (the obscurity of this statement will be challenged below), other crucial elements will also be subject to disbelief; and if this paradigm shift challenges some of the group's members, it will challenge them all.

It is the challenge to these two central assumptions of Kuhn's work, I would suggest, that unites his disparate nonpositivist critics (and indeed, is shared by some of his positivist critics as well), and it is the attempt to qualify these assumptions that characterizes his later responses. In his "Postscript" and his other later work, Kuhn equivocates about the internal consensus of his carrier groups and begins to acknowledge the relative autonomy of the different levels of science. The problem, however, is that he does not give up on his earlier claims altogether. It is in this "neither here nor there" quality of his later writings that the problem really lies.

In Kuhn's "Postscript" (1970:174-210), he tries unsuccessfully to separate the heretofore "necessary" relation he posited between the group and ideational elements of science. That the solution to this basic problem eludes him is clear from the very beginning of his discussion. "A paradigm," he writes, "is what the members of a scientific community share, *and*, conversely, a scientific community consists of men who share a paradigm" (190:176, original italics). But the first clause of this sentence assumes that the members of a scientific community can be decided sociologically, without reference, that is, to their ideational beliefs, while the second clause assumes that the sociological community will be determined by the commitment to the ideational beliefs themselves. Kuhn's key sentence, in other words, is internally contradictory. If the definition of the first clause is followed, science studies move toward an atheoretical, anti-ideational approach which, for example, counts citations and traces "networks" (e.g., Kuhn's statement that "scientific communities can and should be isolated without prior recourse to paradigms," ibid.). The problem with such an approach is that the result, the "paradigmatic community," may be a set of relationships among scientists who share precious little ideational commitment. This is exactly the incongruous situation, for example, that is created by Mullins (1973) in his attempt to delineate the networks that create theory groups in sociology. Though Kuhn approvingly cited Mullins' general network approach in the "Postscript" (1970:176), such a sociologically reductionist approach hardly connotes the united group that he intends his paradigm to imply.

No doubt sensing this problem, Kuhn modifies his claims about the unity of such paradigmatic groups: "Within such groups communication is *relatively* full and professional judgment *relatively* unanimous" (1970:177, italics added). If these italics are not added, of course, the degree to which Kuhn thinks such groups approximate unity is greatly obscured. To judge the degree of unanimity would, indeed, require the independent investigation of the ideational element, and it was no doubt in order to pursue this element that Kuhn included the second clause in his initial investigation.

Kuhn would have his cake and eat it too. When he proposes that one fundamental definition of "paradigm" is synonymous with "disciplinary matrix," he suggests that the investigation of this shared ideational commitment assumes the common group network is already in place. The same contradictory position, in other words, here re-emerges. Kuhn asks, "What do its [i.e., the paradigm group members] share that accounts for the relative fullness of their professional communications and the relative unanimity of their professional judgments?" (1970:182). But he also claims that this cultural analysis of shared ideas should proceed only after "having isolated a particular community of specialists by techniques like those just discussed" (ibid.), i.e., after employing the sociological reduction which contradicts the very basis of the cultural analysis he now seeks to employ.

Kuhn has supported the existence of group consensus so strongly he has endorsed a purely network approach to the organization of scientific beliefs. Yet he has also acknowledged that such unanimity may not actually be produced. To cope with this problem, he has introduced the notion of disciplinary matrix, for it is clear that only an independent focus on the ideational element will predict the degree of group unity, yet, again, he qualified this suggestion by insisting that it is really no different than the earlier sociological reduction. Via this contradictory path the stage is set for Kuhn to confront another fundamental issue that is related to the problem of internal consensus within the scientific group, namely the issue of whether the ideas themselves are inherently interconnected.

If a group of practitioners is to be united and scientific change to be revolutionary, the elements of the scientific belief, must, in fact, be integrally connected. Once again, Kuhn is aware of the weaknesses of this earlier position, but he is, at the same time, no more prepared to give up on the internal cohesiveness of the ideational element than he was prepared to allow the independence of such commitments relative to group ties. It appears, at first, that he is satisfied making only analytical distinctions, and that he does not believe that commitments to these different levels will be the basis of disagreement.

All or most of the objects of group commitment that my original text makes paradigms, parts of paradigms, or paradigmatic are constituents of the disciplinary matrix, and as such they form a whole and function together. They are, however, no longer to be discussed as though they were all of a piece. (ibid.)

He mentions, in this context, symbolic generalizations or laws, definitions, models, methodological values, and problem solutions or exemplars (ibid.:182-7). When discussing these elements, however, it is unclear whether Kuhn believes they do, in fact, "form a whole and function together." Before the Joule-Lenz Law was discovered, he acknowledges, the definitions of its key variables were known well in advance. In the case of Ohm's Law, however, definitions and propositions were inherently intertwined (ibid.:183). The same indeterminateness surfaces in his discussion of models. Not only does he acknowledge that "the strength of group commitment varies, with nontrivial consequences, along the spectrum from heuristic to ontological models," but even within the confines of a particular kind of model scientific unanimity cannot be automatically expected: "the members of scientific communities may not have to share even heuristic models, though they usually do so" (ibid.:184). The same lack of necessary scientific unanimity occurs with the element of methodological values, which turns out to be, like the others, not just analytically differentiated but empirically differentiated as well: "values may be shared by men who differ in their application" (ibid.:185).

What, indeed, has happened to the "functioning together" that, Kuhn earlier insisted, will allow the disciplinary matrix to produce such "relative fullness" of communication and "relative unanimity" of judgment (ibid.:182)? Well might one ask, for what Kuhn has actually demonstrated — equivocally to be sure — is that commitment at any one of these levels does not necessarily go with commitment at any of the others. The "discipline" to which any "matrix" is attached, therefore, is clearly not an internally consistent one, as Toulmin has clearly seen by more modestly defining the critical role of disciplines as allowing comprehensible and continuous communication without mecessarily implying any substantive agreement. For better or worse, it seems clear that a disciplinary matrix cannot generate anything more unanimous than that.

Kuhn failed to justify his insistence on the internal coherence of the ideational element of paradigms much as he failed to do so in his treatment of the sociological or group component. In this case as well, moreover, his failure is demonstrated by the contradictions and equivocations in the very arguments that he himself has produced. The implication of these failures for his insistence on the abrupt and revolutionary character of fundamental scientific change should be clear. If the elements of a theoretical position are not necessarily interconnected, then quite drastic shifts on any given element may not entail the overthrow of the other elements as well. Over time, such shifts, possibly quite radical in themselves, may drastically change the very face of the "same" theory. Such shifts, it should be evident, do not represent the efforts at mere puzzle solving and tidying up that mark the trivial character of normal science: they can be the results of fundamental rethinking that have been undertaken in response to the perception of significant anomalies and to the threat of counter-paradigmatic explanations. If such shifts occur, the result will be revision rather than revolution; in fact, rather than a single scientific theory holding sway, it is quite likely that one will see a plurality of positions whose continuity is assured by the very changes that could, if ideational elements were intertwined, guarantee their elimination.

With this brief exgetical and analytical discussion, I hope I have clarified where my study of "Paradigm revision and 'Parsonianism'" fits vis-à-vis Kuhn's theorizing, in either its early or its later form. Guided by Kuhn's formulations, but motivated as well by the same misconceptions as those which motivated his analysis in the first place, the sociological community has long held "Parsonianism" to be a closely knit paradigm group (for an influential discussion by a Kuhnian, see for example, Friedrichs, 1970; for a non-Kuhnian approach that shares the problem, see virtually any introductory textbook in sociology, even those written by Parsonians themselves). I have demonstrated that this is not the case. Parsons' paradigm supporters differed, and often strongly so, not only with Parsons, but among themselves as well. They differed, moreover, over every conceivable element of the scientific continuum, over epistemological presuppositions, ideology, models, complex propositions, methodology, and even empirical observations.

The life of Parsonianism was not, therefore, a history of conservative puzzle solving, though some followers of Parsons, the less important and now less well-known ones, did confine themselves to the activities of specification and more precise articulation which Kuhn identifies as the lot of normal science. The significant Parsonians were quite radical in their critiques, though often not deliberately so. Challenged by major anomalies and by shifts in the cultural and social climate, these apparently loyal thinkers drastically altered various key elements of Parsons' work. Counter theories emerged in response to similar weaknesses and anomalies in Parsons' theory, but, while they often gained in strength, no revolutionary change ever transpired. The same pattern may be observed in the other empirical and theoretical traditions of sociology — in Marxism, in phenomenology, in conflict and exchange theory — and I would suspect that similar patterns may be observed in the natural sciences as well (for a penetrating discussion of Copernicanism that supports the thesis presented here, see Westman, 1975). In social science at least, revisionism is the order of the day, not revolution. But it is revisionism of a decidedly unconservative type.

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